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# VAGINAL HYSTERECTOMY FOR CANCER.

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Read in the Section on Obstetrics and Gynecology at the Thirty-Sixth Annual Meeting of the American Medical Association.

*Reprinted from the Journal of the American Medical Association, August 15, 1885.*



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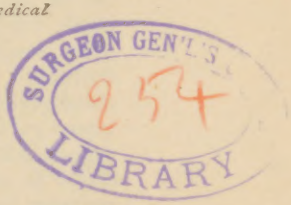


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# VAGINAL HYSTERECTOMY FOR CANCER.

The only fair method of judging the value of any surgical procedure is to consider its results. If these be such as to lessen suffering and prolong life it is useful, and hence proper. If, on the contrary, it fail to effect either of these ends, it is useless. If, further, it be found after sufficient trial to be destructive of life, it is worse than useless—it is injurious, and ought to be abandoned. Judgment should not be rendered prematurely, nor upon insufficient data. Neither should it be based upon theoretical grounds; facts only can be the foundation of an honest opinion.

My purpose in this paper is to apply these principles to the extirpation of the cancerous uterus, with the view of determining what our attitude ought to be in reference to this operation. The abdominal, or Freund's method having, on account of its frightful mortality, been abandoned, or used only in exceptional cases in which removal through the vagina is impracticable, my remarks will have reference wholly to the vaginal operation.

What, then, has vaginal hysterectomy done to deserve our favorable consideration? About three and a half years ago, Hegar and Kaltenbach published a table showing the results of the operation from the date of its revival by Hennig, in 1876, down to the time of the report. It comprised 29 cases, with a mortality of 27.6 per cent. In 1883, Sänger compiled a table which included 133 cases, of which 38, or 28.6 per cent. died. At the annual meeting of the American Gynecological Society, held in Chicago, September, 1884, Dr. Paul F. Mundé stated that he had collected 256 cases, with a mortality of only 24.6 per cent.

Everybody knows that all tables of this character are imperfect, because incomplete. They include only the cases which have been published, or which may possibly have come to the personal knowledge of the compiler. It is equally well understood that the successful cases are far more likely to be published than the others. For example, in my own city (Chicago) the uterus has been removed by the vaginal method four times. One of the patients recovered, and three died. The first was published; the others were not. The single successful case goes into the record showing a recovery rate of 100 per cent.; if its three companions were also considered, the rate would be 25 per cent. Facts of this kind serve to explain a remarkable discrepancy which is apparent when the results of operation as shown by tables of cases gathered from miscellaneous sources, and assumed to be correct, are compared with those of individual operators whose cases are all given to the world. To illustrate: Dr. Engelmann, of St. Louis, in a letter published in the *Weekly Medical Review*, September 1, 1883, stated that down to August 5, Martin had operated by the vagina 50 times, losing one-half. At the meeting of the British Medical Association, in 1883, Prof. Schröder, of Berlin, who had operated in 23 cases, admitted that his mortality was 34 per cent. Ohlshausen, also, in a letter which was read on the same occasion by Sir Spencer Wells, stated that he had operated, or attempted to operate in 28 cases, and his results were equally bad with those of Schröder. The combined number of operations performed by these three men alone was 101—that is, two-fifths of the entire number comprised in Dr. Mundé's table. The mortality in these 101 cases was 38.6 per cent. If Dr. Mundé's data be correct, the operators in the remaining 155 cases were fortunate in having a rate of mortality so preposterously low as to excite wonder. Thus, if the mortality in 256 cases was 24.6

(Mundé), the number of fatal cases was 63. Of these, as we have seen, 39 were included in the 101 cases of Martin, Schröder and Ohlshausen. This leaves 24 other fatal cases to be furnished by the remaining 155, and shows a mortality of 15.4 per cent.!

It seems only just to the gentlemen I have named, to suppose that if all the cases operated upon were published their results would not appear so relatively bad. Indeed, from their well-known skill and experience in pelvic and abdominal surgery we should expect from their work the very best possible success; as would doubtless appear if a good deal of the truth in relation to the subject were not suppressed. Figures will not lie if they have a fair chance.

In view of the tendency to publish successful cases, and the aversion and frequent failure to publish those which are unsuccessful, the conviction is forced upon me that the mortality of vaginal uterine extirpation is not very greatly less than the published mortality of Freund's method, which, in the same manner, was made to appear less than it really was. But, accepting only the published cases, and basing a calculation upon this latest table[1] of 256 cases, with 63 deaths, what is its significance as regards the saving or destruction of life?

While it is impossible to know exactly how many years these 63 women who died would in the aggregate have lived, if permitted, it has been estimated that cancer of the cervix, if left to itself, kills in about seventeen months from the time of the appearance of the distinctive symptoms, and that of the corpus in two and a half years. If we grant that in all these cases the disease affected the cervix, and that the average length of life would be only seventeen months, then the years of life sacrificed would be nearly one hundred. If, in some of them, the disease

[1] A more recent table was compiled by Dr. W. A. Duncan for the Obstetrical Society of London, and published in the Lancet for January 31, 1885. It included 276 cases, with 197 recoveries and 79 deaths—mortality 28.6.

affected the body of the uterus, as it doubtless did, the aggregate amount of life destroyed would be still greater. And if the unreported fatal cases were added, it would be much greater still.

What is there to offset this? Has a single patient been radically cured, and a life otherwise doomed saved by the operation? There may have been, but I do not know that such a claim has yet been made. Schröder[1] has said, "If only one out of twenty be radically cured, this ought to be considered a good result, and as a consolation for many cases treated unsuccessfully." Most surgeons, I fancy, would feel very inadequately consoled by saving one patient and losing nineteen. But here even the one seems lacking. It has been urged that the prognosis of the operation will probably be better when its technical details become more perfect. Unquestionably, it is reasonable to hope that such may be the case, and certainly it is greatly to be desired. But, so far, the history of the operation in this respect is not encouraging. In 1881 Schröder issued a report of eight operations performed by him, with seven recoveries and only one death. After two years' further experience and fifteen additional cases, his percentage of mortality was nearly three times greater.

But, admitting that the operation may by-and-by become less dangerous, what then? Its dangerous character is by no means the gravest charge against it. Indeed, taken alone, it would hardly be a valid one. A far more serious objection to it is that it is not beneficial when it fails to kill. What does it for those who survive? What do they gain? Are they spared the manifold suffering incident to the disease? Are their lives prolonged? Many patients pass from observation after their recovery, and nothing is known of their subsequent history. Probably, however, this does not differ from that of those who continue under notice, and in these recurrence takes place in all,

[1] British Medical Journal, September 15, 1883.

sooner or later. So absolutely is this true that, in any case offering an apparent exception to the statement, the accuracy of the diagnosis may fairly be doubted. Dr. Linkenfeldt, assistant at the clinic at Strasbourg, says that in all cases of total removal of the uterus for cancer performed by Freund relapse followed quickly. In a single instance it happened after two-and-a-half years. In seven cases given by Sänger the average time of recurrence was 4.2 months. Schröder says: "As to the success obtained by vaginal extirpation, I must admit that it is not yet to be called satisfactory, especially as far as the question of recurrence is concerned." And again, "In a great number of my patients I have seen recurrence, in some cases after two or three years. Other cases have withdrawn from my observation, and only very few have remained until to-day quite without recurrence." All candid testimony corroborates this of Schröder.

And now I ask: When recurring symptoms manifest themselves after the temporary diminution or cessation procured by operation, are they in any manner or degree beneficially modified? Are the hæmorrhages less profuse or less exhausting? Is the leucorrhœa less offensive or less irritating? Is the pain less poignant? Does the cachexia kill less certainly? To all of which the answer must come, assuredly not.

Are lives prolonged by the operation? Observations embracing the complete history of those who have recovered from the operation show that they die, on an average, in a shorter time than if let alone.[1] And this is what we might reasonably expect, since so severe an operation is likely to exert a depressing influence upon a person already laboring under an exhausting and deadly disease. Then, if the operation

[1] Sänger (Archiv f. Gynäk., xxxi, 1), states that in six cases observed by him, the average time which elapsed between the operation and death was 14.1 months.

sacrifices many lives and does not save any; if it only postpones or interrupts suffering and does not avert or lessen it, what can be said in its favor? Only this beggarly plea can be urged: that it may in the future not destroy so many, and may possibly even save a life. But should this great and certain evil continue on the mere possibility—the almost baseless hope—that a trifle of good *may* follow? Is this a fair administration of the trust put in us by those who impliedly rely upon our honesty as well as upon our skill?

But, it may be asked, shall no effort be made to save the unfortunate subjects of this dread disease? Unquestionably, it is not only proper, but obligatory upon us to make any and every legitimate endeavor to stay its progress, to mitigate its evils, to avert the threatened death. Such attempts have been made, and are constantly making. But of all the methods employed for these purposes the one which has furnished the worst results, and which has the most utterly failed, is the extirpation of the uterus. Not only is this true of the operation when it is compared with other methods of treatment, but, as already shown, it kills more rapidly than does the disease when allowed to pursue its natural course.

Advocates of the operation are already beginning to place limitations—in theory, at least—upon its employment. Schröder thinks total extirpation ought to be restricted to two classes of cases, namely: 1. Those in which the cancer affects only the body of the uterus; and, 2. Those in which it is limited to the cervical mucous membrane. Dr. Mundé, in the paper to which I have referred, agrees with this, but further states that there should be a sufficiently capacious vagina to permit the ready exposure of the cervix and vaginal vault, and also that the condition of the general system should be sufficiently vigorous to permit the patient to endure the shock of the operation.

These restrictions are eminently proper, so far as they go. They are based upon the fact, admitted by all, that if the disease has at all involved the peri-uterine structures it cannot be wholly removed, and recurrence is certain. The precept is admirable, but the practice does not and cannot correspond with it. The determination of the essential question involved is attended with insuperable difficulty. In the present state of our art there is no method by which the extent of cancerous involvement in a given case can possibly be known. The microscope may sometimes, perhaps generally, tell us of the existence of the disease, but, during life, at least, is certainly incapable of defining its limits. Isolated cancer cells, conveyed by lymphatics and other avenues, are frequently found scattered through the connective and glandular tissues at a considerable distance from the original growth. These outlying areas of affected structure cannot be submitted to microscopical examination prior to operation, the only time when the knowledge obtainable by it could be available for the purpose of deciding the propriety or possibility of radical removal.

Schröder[1] has stated his belief in the feasibility of distinguishing by the touch with perfect certainty very small infiltrations such as occur principally along the lymphatic vessels. I greatly doubt the possibility of making an accurate diagnosis by this means; and the unreliability of the information furnished by it is clearly shown in the rapid and numerous recurrences of the disease after the distinguished gentleman's own operations. When cancer is limited to the cervical mucous lining it is rarely detected, perhaps never. And if it be so limited, the removal of the entire organ is surely not indicated. The excision of a conical portion, including the external os uteri, and extending beyond the internal os, should be sufficient to remove all the diseased tissue. But, I repeat, the

[1] Loc. cit.

disease is not discovered here. At least 90 per cent. of the uterine extirpations have been done for cancer of the cervix; and from the greater frequency, and more easy recognition of the disease in this locality, such will doubtless continue to be the ratio. And here, even the men whose opinions I have cited, and who can hardly be called conservative, regard total extirpation as improper. And for very good reason: When cancer affects the cervix uteri its extension is not upward, towards the body, but circumferential, and supra-vaginal amputation, with a mortality about one-fourth that of total removal, does all that the latter can do.

This, then, practically reduces the field of the operation to cancer of the uterine body. We know less of the disease in this locality than in that of the cervix. As compared with cervical cancer it is much rarer, although of greater frequency than was formerly supposed. Likewise, it is of much longer duration than cervical cancer, and, while confined to the uterine tissues, it is attended by comparatively slight pain. The progress of the disease is by way of the Fallopian tubes and the network of lymphatics which surround the organ on all sides. The difficulties of diagnosis are great, and not likely to be settled until too late to operate. After pain has become a prominent symptom the surrounding tissues are probably involved, and any operation which does not include the tubes and broad ligaments would be incomplete.

Why should a highly dangerous method be chosen in dealing with uterine cancer when others less perilous and equally efficient can be employed? The various operations by the curette, scissors, caustics, cautery, etc., as employed by Sims, Wells, Byrne, Barnes, Van de Warker, Baker, and others, have given results, both immediate and remote, far superior to those of extirpation. In the *Lancet*, for Aug. 2, 1884, there was published an abstract of a valua-

ble report by Pawlik, of 136 cases of cancer of the cervix treated by the galvanic cautery in the first gynecological clinic at Vienna, the observations extending back to 1861. Of the entire number nine died shortly after operation—a mortality of only 6.6 per cent. Not only in regard to the death-rate, but in every other particular the results were incomparably better than those following the erroneously-styled radical method. Indeed, all the minor operative measures to which I have referred, and which are commonly called "palliative," appear to be more radical as regards permanency of cure than total extirpation; and, since their mortality is only about one-fifth of the latter, they should for every proper reason be preferred.

I beg to offer the following conclusions:

1. Any operation for cancer which does not completely remove the disease will be followed by recurrence.

2. During life, the diagnosis of the extent of cancerous disease originating in any part of the uterus, is at present impossible; hence, no operative procedure can afford a guarantee of complete removal.

3. In view of this necessary doubt, no operation is justifiable which greatly endangers life, provided other and safer methods of treatment are available.

4. Vaginal hysterectomy has sacrificed the lives of more than one-third of those who have been subjected to it—the mortality of the operation when done by those of greatest skill and experience being over 36 per cent.

5. Other methods of treatment, attended by not more than one-sixth to one-fourth the mortality of vaginal extirpation, are equally as efficient in ameliorating the symptoms and retarding the progress of the disease; and they have been followed by as good

or better ultimate results. Hence, they should be preferred.

6. Vaginal hysterectomy does not avert or lessen suffering; it destroys, and does not save, life. It is, therefore, not a useful but an injurious operation; and being such, it is unjustifiable, and ought to be abandoned.